

Did Nigeria's 2019 loan-to-deposit ratio mandate deepen private-sector credit? Synthetic-control evidence from African financial systems

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Abstract

Purpose: This study evaluates whether Nigeria's 2019 minimum loan-to-deposit ratio (LDR) mandate was followed by deeper private-sector credit relative to a synthetic no-policy benchmark.

Methodology: Annual World Bank World Development Indicators data are used to construct synthetic Nigeria from 37 African economies. The primary specification fits 2010–2018, treats 2019 as a transition year, and evaluates 2020–2022. In-space placebos, alternative pre-periods, donor restrictions, leave-one-donor-out tests, time placebos, pretreatment holdout validation, and a bank-specific private-credit outcome assess sensitivity.

Results and conclusion: Broad private-sector credit averaged 2.52 percentage points of GDP below synthetic Nigeria during 2020–2022; pre-treatment RMSPE is 0.70 and the post/pre RMSPE ratio is 3.62. ATT-gap ranking gives $p = 0.111$, while fit-normalized RMSPE-ratio inference gives $p = 0.704$. A bank-specific credit robustness check also remains negative (ATT = -2.78 percentage points). The evidence therefore rejects a large, sustained positive aggregate credit-deepening pattern over the observed horizon but does not establish a statistically unusual adverse causal effect.

Implication of findings: System-wide lending floors should not be treated as sufficient evidence-based instruments for durable credit deepening without complementary monitoring and treatment-specific evaluation.

Keywords: loan-to-deposit ratio; private-sector credit; synthetic control; banking regulation; Nigeria

1. Introduction

Credit allocation is a central function of banking systems, particularly in economies where firms depend heavily on intermediate finance. Deep financial systems are associated with stronger capital accumulation and growth, while industries that depend more on external finance tend to grow faster where financial markets are more developed (King & Levine, 1993; Rajan & Zingales, 1998). Policies that force or encourage banks to convert a larger share of deposits into loans therefore have an intuitive appeal. Yet the same intervention can generate offsetting responses: banks may reprice loans, change borrower composition, alter deposit mobilization, substitute across regulated and unregulated balance-sheet channels, or comply mechanically without producing a durable increase in credit relative to the size of the economy. The net aggregate effect is therefore an empirical question rather than a mechanical consequence of the regulatory ratio.

Nigeria provides an unusually important setting for this question. In 2019 the Central Bank of Nigeria (CBN) introduced a minimum loan-to-deposit ratio as an administrative measure to raise lending to the real economy. The Central Bank of Nigeria Communiqué No. 125 of the Monetary Policy Committee Meeting of 22–23 July 2019 and the Central Bank of Nigeria Annual Report 2019 documented a 60 per cent minimum in July 2019, with a 150 per cent weighting for new lending to SMEs, retail, mortgage and consumer segments, and a subsequent tightening to 65 per cent with end-2019 compliance (Central Bank of Nigeria, 2019a, 2019b). The 65 per cent prescribed limit remained relevant during 2020 (Nigeria Deposit Insurance Corporation, 2020). Adenuga et al. (2021) analyzed the intervention and its implications for bank liquidity; the International Monetary Fund (2021) likewise recorded the LDR as an attempt to prop up credit growth and reported limited effectiveness in sequential credit growth. The policy therefore offers a discrete regulatory intervention with a clear directional objective: private-sector

credit should have been higher than it would otherwise have been if the mandate succeeded in deepening credit.

Nigeria entered the 2019 policy period amid several concurrent macroeconomic developments. Oil-price dynamics, exchange-rate pressures, monetary operations, fiscal conditions and, from 2020, the COVID-19 shock changed around the treatment period (International Monetary Fund, 2021). A simple comparison of credit before and after 2019 cannot separate the policy effect from these coincident forces. The identification problem is particularly acute with one treated national banking system and an economy-wide intervention. This study therefore, uses the synthetic control method developed by Abadie and Gardeazabal (2003) and formalized for comparative case studies by Abadie et al. (2010). The method constructs a weighted combination of untreated economies that reproduces Nigeria's pre-policy credit path. Under the usual synthetic-control identification conditions, the post-policy divergence between Nigeria and this data-driven counterfactual provides the estimated policy effect (Abadie, 2021).

The primary outcome is the World Bank World Development indicators series for domestic credit to the private sector as a percentage of GDP (FS.AST.PRVT.GD.ZS). The indicator covers financial resources provided to the private sector by financial corporations through loans and other claims that establish a repayment obligation (World Bank, 2026a). It is deliberately an aggregate financial-depth outcome. Because the LDR mandate directly constrained deposit money banks, the analysis also reports a treatment-aligned robustness check using Domestic credit to private sector by banks (% of GDP), FD.AST.PRVT.GD.ZS, which is defined for other depository corporations rather than the wider financial-corporation perimeter (World Bank, 2026b). The broad measure asks whether economy-wide private-credit depth changed, while the bank-specific check tests whether the main sign depends on the broader measurement perimeter.

The primary design uses 37 African economies with complete broad-outcome observations over 2000–2022 as potential donors. The synthetic-control fit is estimated over 2010–2018. The year 2019 is treated as a transition period because the mandate was introduced during the year and tightened later in the year; 2020–2022 constitute the primary full-treatment window. The resulting synthetic Nigeria is concentrated on Sierra Leone, Mozambique, Egypt and the Republic of Congo. Pre-treatment tracking is close: the root mean squared prediction error (RMSPE) is 0.70 percentage points of GDP. During 2020–2022, actual Nigerian broad private-sector credit averages 2.52 percentage points of GDP below the synthetic counterfactual. The sign remains negative in alternative pre-periods, a Sub-Saharan-Africa-only donor pool, leave-one-donor-out estimations and the bank-specific outcome check.

The paper contributes in three ways. First, it moves the Nigerian LDR literature from associational time-series analysis toward a counterfactual policy-evaluation design. Adenuga et al. (2021), for example, use a factor-augmented VAR framework to examine liquidity consequences up to 2019Q3 but do not construct an untreated post-policy Nigeria. Second, the study distinguishes balance-sheet compliance from aggregate financial deepening. A higher bank-level LDR need not imply that private credit rises relative to GDP if deposits, GDP, loan quality, pricing or non-bank channels also adjust. Third, the analysis is fully reproducible: the open raw data, analysis panel, synthetic weights, placebo calculations, robustness estimates and executable code are documented in the supplementary materials.

The conclusion is intentionally narrower than a claim that the LDR mandate caused credit to fall. The estimated counterfactual gap is economically meaningful and consistently negative, but the finite-sample inference is weak. Ranking comparable-fit average post-treatment gaps gives a one-sided p-value of 0.111, while ranking post/pre RMSPE ratios gives $p = 0.704$. Thus, Nigeria's negative average gap is

toward the lower tail of the gap distribution, but its deterioration in fit is not unusual after normalization by pretreatment fit. Conditional on the synthetic-control assumptions, the evidence is inconsistent with a large, sustained positive aggregate credit-deepening effect over 2020–2022; it does not establish a statistically unusual adverse causal effect.

2. Literature review

Financial development theory provides the broad rationale for policies intended to expand private credit. King and Levine (1993) show that deeper financial systems are strongly associated with subsequent growth, capital accumulation and efficiency improvements across countries. Rajan and Zingales (1998) provide a more mechanism-focused result: industries that are intrinsically more dependent on external finance grow disproportionately faster where financial markets are more developed. These studies do not imply that every increase in credit is welfare improving, but they establish why policymakers in bank-dependent economies may regard private-sector credit depth as a strategically important intermediate outcome.

The regulatory problem is more complicated than the financial-development argument. A minimum LDR is a quantity-based portfolio constraint: holding the denominator and other conditions fixed, a bank below the floor must expand eligible lending, shrink deposits, or otherwise reconfigure its balance sheet to comply. If borrowers are credit constrained and banks possess sufficient capital and liquidity, the first response can increase loan supply. Yet lending mandates also interact with underwriting standards, risk weights, funding costs, reserve requirements and liquidity management. A bank facing a binding ratio may increase high-yield or short-maturity lending, reclassify exposures, ration riskier borrowers through price rather than quantity, or intensify deposit management. The resulting change in aggregate credit depth is therefore not pinned down by the accounting identity of the ratio.

The macroprudential literature reinforces this ambiguity. Lim et al. (2011) document a wide range of quantity, capital and liquidity instruments used across countries and emphasize that effectiveness depends on the instrument, objective and institutional setting. Cerutti, Claessens, and Laeven (2017) find that macroprudential measures are widely used, especially in emerging economies, and are associated with lower credit growth for several instruments. Akinci and Olmstead-Rumsey (2018) similarly report that macroprudential tightening is associated with lower bank and housing credit growth, with targeted instruments often more effective than broad measures. These studies focus mainly on restraint during credit booms; Nigeria's 2019 intervention inverted the conventional direction by using a regulatory floor to stimulate lending. The broader lesson remains: regulatory tools change incentives, but their final credit effect depends on behavioral responses and institutional interactions.

Leakage is one such response. Aiyar, Calomiris, and Wieladek (2014) exploit a United Kingdom policy experiment and show that tighter capital requirements on regulated banks reduced their lending while unregulated branches partly offset the contraction. Although Nigeria's institutional setting differs, the mechanism is relevant: interventions targeted at one balance-sheet perimeter can be diluted by substitution across institutions or financial claims. An aggregate private-credit measure is useful precisely because it does not equate compliance within regulated banks with a net increase in finance received by the private sector.

Nigeria-specific evidence prior to this study is mostly associational or model-based. Adenuga et al. (2021) use quarterly data from 2000Q1 to 2019Q3 and a factor-augmented vector autoregression to study LDR and bank liquidity. They warn that very high LDR requirements can erode liquidity and connect greater lending intensity to inflationary pressure. The IMF's Nigeria Selected Issues report (International

Monetary Fund, 2021) states that the LDR was introduced to prop up credit growth but that its effectiveness had been limited, as reflected in declining sequential credit growth; the same report also identifies weaknesses in the broader monetary-policy transmission framework. NDIC banking-industry evidence shows that the net credit-to-deposit ratio was 63.04, 60.97 and 59.56 per cent in the second, third and fourth quarters of 2020, respectively, below the 65 per cent prescribed limit (Nigeria Deposit Insurance Corporation, 2020). These observations underscore that a statutory floor does not mechanically translate into persistent observed industry-level compliance or into a durable increase in aggregate credit depth.

Recent FUJAFR banking studies also show the journal's emphasis on Nigerian financial-sector mechanisms and policy relevance. Andortan Andortan, Effiong, and Okon Ikpe (2026) use CBN annual data to link cashless-policy channels to deposit-money-bank performance, while Suotor, Ekpote, and Ekpote (2026) examine banking-sector innovations and manufacturing-sector performance. Abubakar, Abdullahi, Suleiman, and Abdullahi (2026) examine Basel III regulatory reforms and financial performance in Nigerian deposit money banks; Oyamendan, Adebolanle, Asaolu, and Afolabi (2026) examine bank-specific risk governance and regulatory compliance; and Obaka and Sunday (2025) examine enterprise risk management, capital adequacy risk, and non-performing-loan risk in Nigerian deposit money banks. The present study complements this stream but changes the identification strategy: rather than asking whether two Nigerian time series co-move, it estimates the missing counterfactual path of Nigeria's private-credit depth after a discrete regulatory intervention.

A credible synthetic-control design requires more than a visible post-treatment gap. Abadie (2021) emphasizes strong pre-treatment fit, a donor pool not exposed to the intervention, absence of anticipation or spillovers that invalidate control units, and transparent sensitivity analysis. With a single treated aggregate unit, inference is naturally based on placebo assignments rather than large-sample standard errors. The current design therefore treats pre-treatment fit, alternative placebo statistics, time-placebo tests and sensitivity analysis as core evidence rather than optional diagnostics.

The policy's stated direction yields a simple testable expectation. If the minimum LDR successfully deepened private-sector finance at the aggregate level, Nigeria should display a positive post-policy gap relative to a synthetic counterfactual that reproduces its pre-policy trajectory. Formally, H1 is that the average treatment effect on domestic credit to the private sector over the full-treatment years 2020–2022 is greater than zero. Because the analysis is a policy evaluation rather than a conventional regression exercise, failure to support H1 does not imply that no individual bank expanded lending; it implies that the aggregate financial-depth outcome did not rise relative to the estimated no-policy path.

3. Methodology

3.1 Study Design

The study adopts a comparative case-study design with Nigeria as the treated unit and African countries as potential controls. The intervention is the CBN minimum LDR mandate introduced in 2019 (Central Bank of Nigeria, 2019a, 2019b). Because implementation began during the year and the minimum was subsequently tightened, 2019 is not treated as a clean full-treatment year. The primary pre-treatment period is 2010–2018 and the primary treatment-effect window is 2020–2022. This timing reduces mechanical contamination of the pre-period while recognizing that annual data cannot isolate the exact month of implementation. A transition-inclusive estimate for 2019–2022 is reported as a secondary result. Table 1 summarizes the research design, treatment timing and data architecture.

Table 1. Research design and data architecture

Element	Specification
Treated unit	Nigeria
Policy intervention	Minimum loan-to-deposit ratio mandate introduced in 2019
Outcome	Primary: Domestic credit to private sector (% of GDP), WDI FS.AST.PRVT.GD.ZS; robustness: bank-specific credit, FD.AST.PRVT.GD.ZS
Data frequency	Annual
Balanced panel	38 African economies, 2000–2022
Potential donors	37 economies
Primary pre-period	2010–2018
Transition year	2019
Primary post-period	2020–2022
Estimator	Convex synthetic control; weights ≥ 0 and sum to 1
Inference	Corrected in-space placebos; ATT-gap, absolute-gap and post/pre RMSPE-ratio ranks
Open-data license	World Bank WDI CC BY 4.0

Source: Authors' design using World Bank World Development Indicators and publicly documented CBN policy timing.

3.2 Data

The primary outcome is World Bank WDI indicator FS.AST.PRVT.GD.ZS, Domestic credit to private sector (% of GDP). The World Bank defines the series as financial resources provided to the private sector by financial corporations through loans and other claims that establish a repayment obligation; the series draws on IMF International Financial Statistics and World Bank/OECD GDP and national-accounts data and is annual (World Bank, 2026a). The archived broad-outcome CSV was obtained from a public WDI mirror, while indicator identity, definition, source and licence were cross-checked against official World Bank metadata. To address treatment-measurement alignment, a robustness analysis uses FD.AST.PRVT.GD.ZS, Domestic credit to private sector by banks (% of GDP), which covers other depository corporations and is therefore closer to the institutions directly constrained by the LDR mandate (World Bank, 2026b). The bank-specific robustness archive uses public World Bank-series values exposed through a reusable data mirror, with the positive-weight donor observations retained at source precision and remaining donor observations archived at the displayed precision; this check is explicitly treated as a measurement-validity sensitivity analysis rather than a replacement for the higher-precision primary archive.

The primary donor pool is constructed from African economies with complete broad-outcome observations for 2000–2022. Thirty-eight economies, including Nigeria, meet this completeness rule, leaving 37 potential donors. The rule uses post-treatment data availability to maintain a balanced panel, but post-treatment outcome magnitudes are not used in the primary weight optimization. Weights are selected solely to reproduce Nigeria's 2010–2018 outcome trajectory. The primary estimator is deliberately outcome-path based and does not add macroeconomic predictors that might be inconsistently measured or post-policy affected. Its nine annual fitting observations and relatively large donor set make placebo, holdout and sensitivity diagnostics particularly important.

3.3 Synthetic-control estimator

Let $Y(N,t)$ denote Nigeria's observed private-credit depth in year t and $Y(j,t)$ the outcome for donor country j . Synthetic Nigeria is the convex weighted average of donor outcomes, using non-negative

donor weights constrained to sum to one, as specified in Equation (1). The weight vector minimizes the mean squared Nigeria–synthetic outcome gap over 2010–2018, as shown in Equation (2). For each post-policy year, the estimated treatment effect is defined as Nigeria's observed outcome minus the synthetic no-policy outcome. The primary estimand is the arithmetic mean of those annual effects for 2020, 2021 and 2022, as defined in Equation (3). Negative values therefore mean observed Nigerian private-credit depth is lower than its synthetic no-policy path.

$$Y^S_t = \sum_{j=1, \dots, J} w_j Y_{jt}, \quad w_j \geq 0, \quad \sum_{j=1, \dots, J} w_j = 1. \quad (1)$$

$$w^* = \arg \min_w (1/T_0) \sum_{t \in T_0} [Y_{Nt} - \sum_{j=1, \dots, J} w_j Y_{jt}]^2. \quad (2)$$

$$\hat{\tau}_t = Y_{Nt} - Y^S_t; \quad ATT = (1/3) \sum_{t=2020, \dots, 2022} \hat{\tau}_t. \quad (3)$$

The convexity restrictions improve transparency and guard against extrapolating outside the donor support. They also allow the synthetic unit to be interpreted as a weighted portfolio of real economies. As Abadie et al. (2010) and Abadie (2021) stress, the quality of the counterfactual depends critically on pre-treatment tracking. The principal fit statistic is the pre-treatment RMSPE, defined in Equation (4) as the square root of the mean squared Nigeria–synthetic gap over 2010–2018. A second descriptive statistic is the ratio of post-treatment RMSPE to pre-treatment RMSPE; a large increase signals that the treated unit departs from its prior fit after the intervention, although this ratio alone is not a formal p-value.

$$RMSPE = \{ (1/T) \sum_t [Y_{Nt} - Y^S_t]^2 \}^{(1/2)}. \quad (4)$$

3.4 Inference

With one treated country and a short post-treatment horizon, conventional regression standard errors are not appropriate. The study therefore applies in-space placebo tests. Each untreated country is reclassified in turn as if it had received the Nigerian policy in 2019; when a control country is placebo-treated, actual Nigeria is excluded from its donor pool so the treated unit cannot contaminate placebo counterfactuals. Comparable-fit inference uses placebo units with pre-treatment RMSPE no more than twice Nigeria's. Two complementary statistics are reported: (i) the one-sided rank of the average post-treatment gap, which asks how often a placebo gap is at least as negative as Nigeria's; and (ii) the rank of the post/pre RMSPE ratio, which normalizes post-treatment divergence by pretreatment fit. A two-sided absolute-gap rank is also reported. The alternative statistics answer different questions and are presented together rather than selecting the most favorable one. The one-sided placebo rank is formalized in Equation (5).

$$p_{\text{one-sided}} = \#\{i: ATT_i \leq ATT_{\text{Nigeria}}\} / N_{\text{comparable}}. \quad (5)$$

3.5 Robustness and validation analyses

The robustness checks summarized in Table 5 include longer pre-treatment windows (2007–2018 and 2000–2018), a Sub-Saharan-Africa-only donor pool, leave-one-positive-donor-out estimations, and the transition-inclusive 2019–2022 average gap. Additional validation addresses three specific threats identified by the design. First, a bank-specific private-credit outcome (FD.AST.PRVT.GD.ZS) tests whether the main sign depends on the broader financial-corporation measure. Second, pseudo-transition dates in 2014 and 2015 test whether similarly large fit deterioration appears before the actual intervention. Third, a pretreatment holdout exercise fits 2000–2014 and evaluates out-of-fit prediction over 2015–2018. These analyses are reported sensitivity tests; no claim of preregistration or formal prespecification is made.

3.6 Identification assumptions and threats

The causal interpretation rests on five substantive conditions. First, absent the LDR mandate, Nigeria would have continued to evolve approximately like the weighted donor combination after accounting for common shocks. Second, donor countries did not receive a sufficiently similar contemporaneous intervention that would contaminate the counterfactual. Third, Nigerian policy spillovers to donor outcomes are negligible. Fourth, no Nigeria-specific shock beginning at the same time fully explains the divergence. Fifth, outcome measurement is sufficiently comparable over time and countries. These assumptions cannot be proven from annual country data. Credibility is assessed through pretreatment fit (Equation (4)), placebo comparisons (including the one-sided rank in Equation (5)), fit-normalized inference, time placebos, holdout validation, alternative samples and the bank-specific outcome check. The COVID-19 period and other Nigeria-specific macroeconomic shocks remain material residual threats, so results are interpreted as quasi-causal counterfactual evidence rather than definitive causal proof.

3.7 Reproducibility and research ethics

All observations are aggregate, public secondary data. No human participants, personal data, confidential bank records or constructed pseudo-observations are used. Replication materials contain the archived primary raw file, bank-specific robustness data, balanced analysis panel, synthetic weights, annual effects, corrected placebo results, time-placebo and holdout diagnostics, robustness outputs, figures, executable Python code and a supplementary workbook. The analysis does not impute missing primary-outcome values.

4. Results and discussion

4.1 Descriptive pattern and pre-treatment fit

Nigeria's private-sector credit depth averaged 12.31 per cent of GDP during the primary 2010–2018 fitting period, with values ranging from 10.25 to 14.61 per cent. The purpose of the synthetic control is not to reproduce the donor-pool average but to reproduce this specific trajectory. The optimized synthetic combination assigns 58.47 per cent weight to Sierra Leone, 21.73 per cent to Mozambique, 13.82 per cent to Egypt and 5.97 per cent to the Republic of Congo. All other donors receive effectively zero weight. The pre-treatment RMSPE is 0.704 percentage points, and annual pre-policy gaps alternate around zero rather than displaying a systematic trend. This fit is strong enough to make post-treatment divergence substantively interpretable. Table 2 reports the resulting donor weights and pre-treatment fit statistics.

Table 2. Primary synthetic-control weights and pre-treatment fit

Code	Donor economy / metric	Weight / value
SLE	Sierra Leone	0.5847
MOZ	Mozambique	0.2173
EGY	Egypt, Arab Rep.	0.1382
COG	Congo, Rep.	0.0597
—	Pre-treatment RMSPE	0.7042

Note: Only positive weights above numerical tolerance are displayed. RMSPE is in percentage points of GDP. **Source:** Authors' calculations from World Bank World Development Indicators.

Figure 1 plots Nigeria and synthetic Nigeria from 2010 through 2022. The two series move closely before treatment, including the rise through 2016 and the decline during 2017–2018. A pronounced separation appears in 2019. Nigeria records private-sector credit equal to 7.92 per cent of GDP compared with 10.79 per cent for the synthetic counterfactual, a gap of –2.87 percentage points. Because 2019 is the transition

year, this observation is not included in the primary ATT; nevertheless, its timing matters for interpretation. The gap predates the first full pandemic year, reducing—without eliminating—the concern that the entire result is a Nigeria-specific COVID-19 artifact.

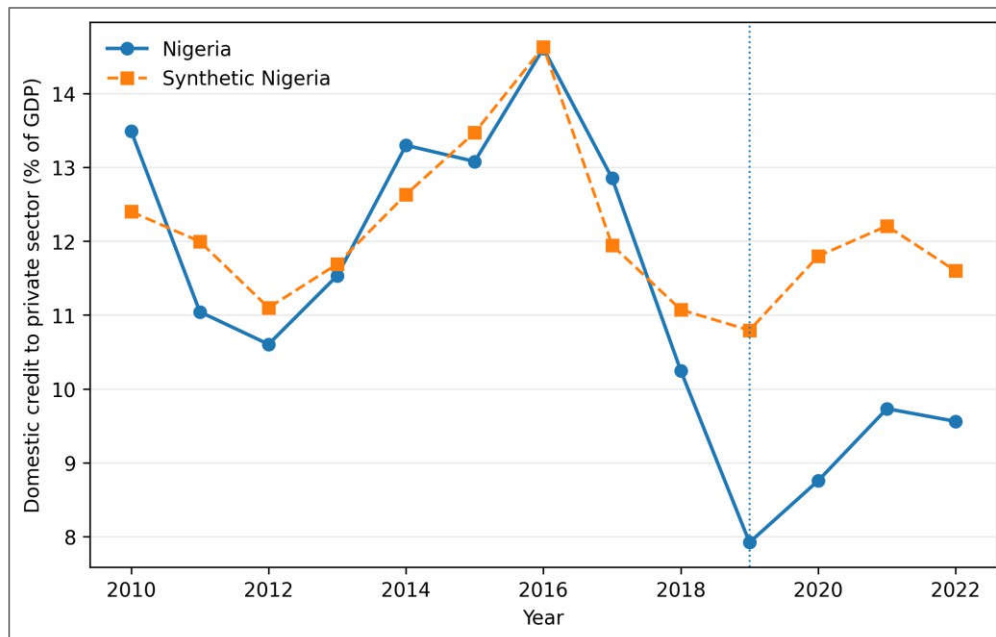


Figure 1. Nigeria and synthetic Nigeria: domestic credit to private sector (% of GDP), 2010–2022

Note: Vertical dotted line marks the 2019 transition year. *Source:* World Bank WDI; authors' calculations.

4.2 Primary treatment effect

In 2020, Nigeria's private-sector credit depth is 8.76 per cent of GDP versus 11.80 per cent for synthetic Nigeria, producing a gap of -3.04 percentage points. The gap remains negative in 2021 (-2.47) and 2022 (-2.04). Averaged over the three full-treatment years, the estimated effect is -2.52 percentage points of GDP. Relative to the synthetic counterfactual's 2020–2022 mean of about 11.87 per cent, the absolute gap is economically large. The post-treatment RMSPE is 2.55 percentage points, yielding a post/pre RMSPE ratio of 3.62. Table 3 reports the annual treatment-effect estimates, while Figure 2 plots the Nigeria-minus-synthetic gap over time.

Table 3. Primary annual treatment-effect estimates

Year	Nigeria	Synthetic	Gap	Classification
2018	10.247	11.072	-0.826	Pre-treatment
2019	7.923	10.794	-2.871	Transition
2020	8.760	11.798	-3.038	Post-treatment
2021	9.733	12.206	-2.473	Post-treatment
2022	9.562	11.598	-2.036	Post-treatment
2020–2022			-2.516	Average full-treatment effect

Note: Gap = Nigeria minus synthetic Nigeria, in percentage points of GDP. Negative values indicate lower observed credit depth than the counterfactual. *Source:* Authors' calculations from World Bank World Development Indicators.

The sign directly contradicts H1, the hypothesis that the mandate would increase aggregate private-credit depth relative to the no-policy path. This does not mean that regulated banks failed to originate additional loans or that every component of private credit declined. It means that, after controlling for

the pre-treatment path through synthetic comparison, the broad amount of private credit relative to GDP was lower than the counterfactual over the observed full-treatment window. The distinction is important because LDR compliance can be achieved by multiple balance-sheet responses and because an annual credit-to-GDP ratio embeds both credit and GDP movements.

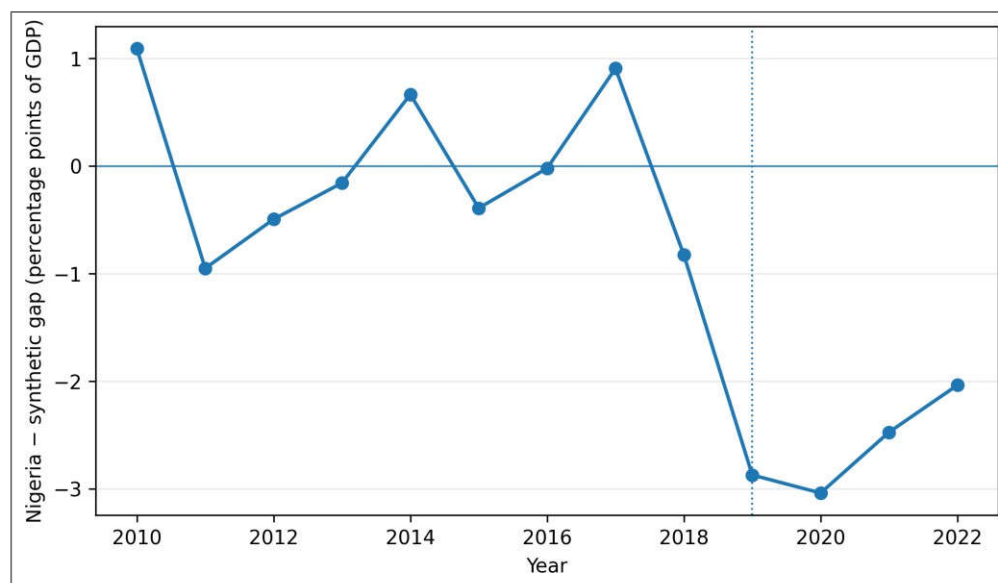


Figure 2. Nigeria-synthetic treatment gap

Source: World Bank WDI; authors' calculations.

4.3 Placebo inference

Twenty-seven cases, including Nigeria, satisfy this comparability rule. Nigeria is the third most negative average effect in that group, giving an ATT-gap one-sided p-value of $3/27 = 0.111$; the two-sided absolute-gap p-value is 0.370. However, post/pre RMSPE-ratio inference is substantially weaker: 19 of the 27 comparable-fit cases have an RMSPE ratio at least as large as Nigeria's 3.62, giving $p = 0.704$. Thus, Nigeria's average gap is relatively negative, but the post-policy deterioration in fit is not statistically unusual once normalized by pretreatment fit. Table 4 reports all three inferential summaries.

Table 4. Placebo-in-space inference

Statistic	Result
Nigeria pre-treatment RMSPE	0.704
Nigeria average post gap, 2020–2022	–2.516 pp GDP
Comparable-fit cases ($\text{pre-RMSPE} \leq 2 \times \text{Nigeria}$)	27
Rank in negative tail	3rd most negative
One-sided randomization-style p-value	0.111
Two-sided absolute-gap p-value	0.370
Post/pre RMSPE-ratio p-value	0.704

Interpretation: The ATT-gap ranking places Nigeria toward the negative tail, but fit-normalized RMSPE-ratio inference is weak ($p = 0.704$). The evidence does not establish conventional statistical significance or an unusually large deterioration in fit. **Source:** Authors' corrected placebo calculations from World Bank World Development Indicators.

The combined inference is deliberately conservative. A graphically large negative gap does not by itself establish a policy effect, and the two placebo statistics show why. Nigeria ranks toward the negative tail

when average gaps are compared, yet many placebo units experience equally large or larger post/pre fit deterioration. Accordingly, the evidence is useful for rejecting a strong positive credit-deepening narrative over the observed horizon, but it is insufficient to claim that the mandate definitively caused an adverse aggregate-credit effect.

4.4 Robustness and validation

As shown in Table 5, the 2020–2022 ATT remains negative when the broad-outcome fit begins in 2007 (ATT = -4.58), uses 2000–2018 (-3.47), restricts donors to Sub-Saharan Africa (-2.08), or removes each positive-weight donor in turn (range: -4.38 to -2.08). The transition-inclusive 2019–2022 average gap is -2.60. Importantly, a treatment-aligned bank-specific credit outcome also produces a negative 2020–2022 gap: ATT = -2.78 percentage points of GDP, pre-RMSPE = 0.71 and post/pre RMSPE ratio = 4.01. This bank-specific series is treated as a measurement-alignment sensitivity analysis; the replication script includes an optional direct World Bank Indicators API refresh so that a uniform-precision official extract can be regenerated when network access is available. Table 5 summarizes the principal sensitivity estimates.

Table 5. Robustness of the 2020–2022 average treatment effect

Specification	Pre-period	Donors	Pre-RMSPE	ATT (pp GDP)
Primary	2010-2018	37	0.704	-2.516
Longer pre-period	2007-2018	37	1.834	-4.583
Full pre-period	2000-2018	37	2.351	-3.472
SSA-only donors	2010-2018	33	0.822	-2.078
Leave out Sierra Leone	2010-2018	36	0.901	-4.380
Leave out Mozambique	2010-2018	36	0.852	-3.275
Leave out Egypt	2010-2018	36	0.822	-2.078
Leave out Congo, Rep.	2010-2018	36	0.713	-2.332
Bank-specific credit outcome	2010-2018	15	0.715	-2.777

Note: The bank-specific row uses WDI indicator FD.AST.PRVT.GD.ZS and a 15-country African donor set with complete 2010–2022 values in the archived robustness file. *Source:* Authors' calculations from World Bank-series data.

Two additional diagnostics address timing and overfit concerns. Backdating the intervention produces much smaller fit deterioration: a 2014 pseudo-transition gives a 2015–2017 average pseudo-gap of -0.60 percentage points and an RMSPE ratio of 0.27; a 2015 pseudo-transition gives a 2016–2018 average pseudo-gap of -1.29 and a ratio of 0.76. Both are well below the actual post-policy ratio of 3.62. A separate holdout exercise fits 2000–2014 and predicts untreated 2015–2018 observations; holdout RMSPE (1.72) is lower than training RMSPE (2.55), with a holdout/training ratio of 0.67. These checks reduce concern that an equally sharp break is routinely generated before treatment, although they do not eliminate contemporaneous post-2019 confounding.

4.5 Why the lending floor may have failed to raise aggregate credit

One possible explanation is that the policy affected the composition of bank assets more than the economy-wide stock of private claims. Banks can comply by reallocating toward eligible assets, managing deposits, repricing credit or concentrating on borrowers that satisfy risk-return constraints. Interaction with other monetary and liquidity instruments provides another explanation. The IMF (2021) reports that the LDR was intended to prop up credit growth but had shown limited effectiveness in sequential credit growth, while also documenting weaknesses in Nigeria's broader monetary-policy transmission framework. Leakage or substitution across the financial perimeter is also relevant,

analogous in logic to Aiyar et al. (2014): a regulatory impulse within one institutional segment need not translate one-for-one into aggregate financing. Macroeconomic risk provides a further explanation. If borrower quality deteriorates or uncertainty rises, a quota can raise the shadow cost of safe compliance without creating bankable investment demand.

The findings are consistent with a broader lesson from macroprudential research: instrument-objective mapping matters. Akinci and Olmstead-Rumsey (2018) and Cerutti et al. (2017) show that different instruments can have different effects on targeted credit aggregates. Nigeria's intervention was expansionary rather than restrictive, and the present design does not compare the LDR floor with alternative regulatory tools. The evidence therefore supports caution about treating a system-wide LDR floor as sufficient for durable credit deepening; it does not establish that any particular alternative instrument would have performed better.

4.6 Validity and limitations

Five limitations bound the claims. First, the primary outcome covers private credit provided by financial corporations rather than bank loans alone; the bank-specific robustness check addresses this mismatch partially and retains a negative sign, but its smaller donor archive is a sensitivity analysis rather than a substitute for granular bank-level treatment exposure. Second, the primary synthetic control is fitted to nine annual pre-policy observations using a 37-country donor pool and an outcome-only specification. Placebos, alternative windows, leave-one-out tests and pretreatment holdout validation reduce but do not eliminate overfitting or structural-comparability concerns. Third, annual data provide only three full post-treatment observations through 2022. Fourth, 2020–2022 includes the pandemic and other Nigeria-specific shocks; time placebos show no comparably large pre-2019 fit break, but country-specific post-treatment confounding remains possible. Fifth, finite-sample inference is weak: the ATT-gap rank is $p = 0.111$ and the RMSPE-ratio rank is $p = 0.704$.

A further measurement caveat is the denominator. Credit-to-GDP can decline because nominal credit falls, GDP rises faster, or both. The outcome is nevertheless appropriate to the stated concept of financial depth and is widely used for cross-country comparison. Future work with public monthly or quarterly bank-level data could decompose volumes, sectoral allocation, pricing, maturities, non-performing loans and deposit responses, and could exploit bank-specific exposure to the pre-policy LDR gap in a difference-in-differences framework. That design would complement rather than replace the present aggregate counterfactual evidence.

5. Conclusion

This study evaluates whether Nigeria's 2019 minimum loan-to-deposit ratio mandate was followed by a sustained increase in private-sector credit depth relative to a synthetic no-policy benchmark. In the primary broad-credit specification, Nigeria tracks its synthetic counterpart closely before the policy but averages 2.52 percentage points of GDP below it during 2020–2022. Alternative broad-outcome specifications preserve the negative sign, and a bank-specific private-credit robustness check yields an ATT of -2.78 percentage points.

The evidence does not support the policy's positive aggregate credit-deepening hypothesis over the observed horizon, but neither does it establish a statistically unusual adverse causal effect. The ATT-gap placebo rank is $p = 0.111$, while the fit-normalized RMSPE-ratio rank is $p = 0.704$. Time-placebo and holdout diagnostics are reassuring about pre-treatment instability, yet contemporaneous post-2019 shocks remain possible. The most defensible conclusion is therefore that a large, sustained positive credit-

deepening effect is not visible in these data; the negative counterfactual divergence should be treated as economically meaningful but causally uncertain.

Three implications follow. First, policymakers should not treat compliance with a system-wide lending floor as sufficient evidence that aggregate private credit has deepened; treatment-specific outcome monitoring and independent counterfactual evaluation are needed. Second, the CBN and other financial authorities should publish granular bank-level data on pre-policy LDR exposure, loan origination by sector and firm size, pricing, maturities, deposits and non-performing loans so that mechanisms and heterogeneous treatment effects can be tested directly. Third, the interaction of lending incentives with reserve, liquidity, capital and monetary-policy settings should be evaluated jointly because one instrument can offset or redirect another. The present study does not rank alternative policy instruments and therefore makes no claim that a particular substitute would necessarily perform better.

The analysis also illustrates the value – and limits – of transparent counterfactual methods for financial-policy questions in emerging markets. The negative point estimate is stable across several specifications, while the fit-normalized placebo evidence requires substantial restraint. Publishing the data, code, corrected placebo design, alternative outcome and validation checks makes these distinctions auditable and gives later work a clear basis for stronger bank-level or higher-frequency identification.

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Appendix A. Balanced African analysis panel

Code	Country/economy	Code	Country/economy
BDI	Burundi	LBR	Liberia
BEN	Benin	KEN	Kenya
BWA	Botswana	GNQ	Equatorial Guinea
BFA	Burkina Faso	ZAF	South Africa
CAF	Central African Republic	TZA	Tanzania
CIV	Cote d'Ivoire	TUN	Tunisia
GIN	Guinea	STP	Sao Tome and Principe
COD	Congo, Dem. Rep.	SWZ	Eswatini
COG	Congo, Rep.	TGO	Togo
COM	Comoros	MLI	Mali
DJI	Djibouti	MOZ	Mozambique
CPV	Cabo Verde	RWA	Rwanda
DZA	Algeria	MUS	Mauritius
EGY	Egypt, Arab Rep.	MWI	Malawi
GHA	Ghana	NER	Niger
GNB	Guinea-Bissau	NGA	Nigeria
MDG	Madagascar	SLE	Sierra Leone
LSO	Lesotho	SEN	Senegal
LBY	Libya	SDN	Sudan

Note: All listed units have complete FS.AST.PRVT.GD.ZS observations for 2000–2022. Nigeria is the treated unit; the remaining 37 form the primary donor pool. Eligibility uses complete-data availability over the panel window; primary weights are optimized only on 2010–2018 outcome magnitudes.